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Preparations and Principles

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The basic principles of behavior analysis have been discovered through research on the behavior of nonhuman animals. Do those principles apply also to the behavior of humans? That question, according to Baron, Perone, and Galizio has not been answered satisfactorily but must be answered in order to rebut the critics of behavior analysis and for behavior analysis to be taken more seriously by nonbehavioral psychologists. Baron et al. argue, further, that free-operant preparations where adult, verbal humans receive points as a consequence of pressing buttons—the human Skinner box—will prove to be invaluable for answering the questions about the generality of basic principles related to the reinforcement process.

Whether or not a particular experimental preparation will turn out to be useful cannot, of course, be known in advance. Our hunch, however (Shull et al., 1989), was that the human Skinner box preparation is not likely to be a good one for answering questions about the fundamental effects of operations on response rate. And despite Baron et al.'s defense, we remain skeptical.

There are several issues that seem to underlie our disagreement with Baron et al. First, whether or not one thinks a particular experimental preparation will be suitable for revealing fundamental relations will depend partly on what one means by *fundamental.* And Baron et al. seem to mean something different by *fundamental* from what we mean. Second, we think Baron et al. have not given enough attention to why one experimental preparation is better than other imaginable preparations that might be appropriate. And, finally, we have been unable to tell what specific doubts held by nonbehavioral psychologists Baron et al. think are likely to be addressed by the research they advocate.

Our previous criticisms were not directed toward any experimental preparation per se. Indeed, our point was that preparations cannot be evaluated independently of the particular question about behavior that the researcher is trying to answer. That is, we thought the question should determine the preparation rather than the other way around. Our fear, quite frankly, was that some behavior analytic research with human subjects will appear to be method-driven instead of problem-driven (Platt, 1964) unless those who conduct such research make the nature and significance of the questions more visible. In the hope of prompting further discussion of these matters, we offer the following additional comments.

FUNDAMENTAL RELATIONSHIPS AND EXPERIMENTAL PREPARATIONS

Fundamental Relationships

Baron et al. argue that the generality of fundamental reinforcement processes needs to be more firmly established. But what are those fundamental processes? Do they mean by *fundamental* those relationships that hold over the widest range of circumstances (i.e., that are the most general) and thus that are the foundation of a scientific system? If so, we agree. Complex phenomena are analyzed as products of combinations of such foundational relationships and their particular values (cf. Catania, 1983; Sidman, 1986). It is obviously important to determine whether or not the relationships that behavior analysts regard as fundamental are in fact so.

But do the patterns of behavior engendered by different reinforcement schedules exemplify fundamental relation-

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ships? Sometimes Baron et al. write as if they think so. True, the patterns occur consistently and fairly generally. But the relationships involving patterns are not the foundation of any system that we are aware of.

Instead, as Baron et al. well understand, the patterns of behavior engendered by reinforcement schedules are usually conceptualized as the result of combinations of more fundamental variables arranged by the current and previously experienced environments (Ferster & Skinner, 1957; Morse, 1966; Zeiler, 1977). Those variables are more fundamental in the sense of operating the same way across schedule type and situation. That is, they and their effects are the elements, the foundation, of a behavioral analysis.

Differences in performance under the same current schedule can result from small, quantitative differences in the effects of one or more of those variables as well as from more substantial differences in the types of controlling variables. Conversely, similar patterns can arise from very different combinations of fundamental variables.

No doubt careful experimental analyses can be undertaken to reveal the bases of similarities and differences. But are such analyses likely to reveal relationships that are sufficiently foundational, or general, to be of broad interest? Or are such analyses likely to show instead that particular complex arrangements of current and remote fundamental variables produce particular complex outcomes? Until such questions are addressed more directly, we are likely to remain unsure about what significance to attach to whatever empirical generalizations emerge about similarities and differences in performance under reinforcement schedules between human and nonhuman subjects.

Experimental Preparations

The mere fact that research is done in a laboratory where certain conditions are controlled does not mean that the experimental preparation has effectively isolated the *relevant* variables needed to discover fundamental laws. Skinner (1938) made much the same point in discussing why he favored the rat's lever press. His reasoning is worth recalling.

For Skinner, the essential property of operant behavior to account for was its likelihood (i.e., its rate or probability) of occurrence (1938, p. 46; 1950, pp. 197–199; 1953, pp. 62–63; 1957, pp. 22–33). Thus, *fundamental* principles were those that specified the most general functional relationships between classes of operations and the probability, or rate, of operant behavior.

Observed relationships seemed unlikely to be general unless the response was free to vary over a wide range of rates and unless the response was relatively unconstrained by other variables (e.g., Skinner, 1953, pp. 62–63). For example, because of constraints due to social contingencies, the controlling relationships that Skinner regarded as fundamental would not normally be apparent in the rate of ongoing verbal behavior (Skinner, 1957, pp. 22–28). The fundamental relationships operated, Skinner argued, but in complex combinations.

Skinner (1938) believed that the rat's lever press, in contrast, was relatively unconstrained because it "is not included in any other significant behavior" (p. 49). It thus seemed "nearly optimal" (p. 50) for determining the effects of variables on response probability.

Significantly, Skinner (1938) did not think all simple-appearing responses of a rat were equally suitable:

> The response of flexing a foreleg, for example, might be a component part in the responses of scratching, eating, cleaning the face, running, climbing, and so on. A description of its changes in strength would need to take all these various behaviors into account. (pp. 49–50)

Although Skinner (1938) believed that such responses were lawfully related to their controlling variables, he stressed that the complex combinations of variables would make the discovery of lawfulness "more difficult" (p. 50):

> For example, if the response were part of many different kinds of conditioned and unconditioned behavior, the curves obtained during various

changes in strength would be composite and highly complex. (p. 51)

Presumably, if Skinner had thought lever pressing "were part of many different kinds of conditioned and unconditioned behavior"—as he believed foreleg-flexion to be—he would have chosen some other response to study. Actually, the rat's lever press probably is not as ideal a preparation for his purposes as Skinner once believed. For example, the differential reinforcement of response rate can obscure fundamental relations between response rate and classes of controlling variables.

But questions about the value of the rat's lever press as an experimental preparation are of secondary relevance to the present discussion. It is centrally relevant, however, that a button-press by an adult, verbal human might be more like what Skinner imagined a rat's foreleg-flexion to be than what Skinner imagined a rat's lever-press to be. That is, a human's button press seems likely to be part of many different behavior classes, including such complex, higher-order classes as "strategies" for interacting with games, complex chains prompted by and prompting verbal behavior, and "test-taking" repertoires reinforced and motivated by signs of social approval and success (see Dinsmoor, 1983; Michael, 1987; Shimoff, Matthews, & Catania, 1986). If so, observed relationships involving response rate are likely to be "composite and highly complex."

There is a related concern about the use of points as reinforcement. Although the delivery of points for humans might appear analogous to the delivery of food to a rat or pigeon, the similarities might be somewhat superficial and misleading (Case, Ploog, & Fantino, 1990, p. 197). For example, the conditions typically used to make points effective include instructions and other complex social relationships (e.g., the subject must "understand" and "trust" the experimenter). Consequently, points might serve various stimulus functions in addition to any reinforcement function derived from the back-up reinforcers.

Perhaps these other sources of control can be weakened sufficiently by the methods that Baron et al. describe, such as extended training. But when we remember the extensive social histories that humans likely have had relevant to performance in game-like and test-like situations and when we think of the practical constraints on human research, we find it hard to be optimistic. Furthermore, the achievement of stability after extended training does not necessarily imply that other sources of control have been weakened. It may occur because of the dominance of some source of control other than what the experimenter intended (e.g., "rules" rather than response-consequence relations).

If our hunch is right about these kinds of complexities, would that mean the human Skinner box preparation is incapable of revealing fundamental relations? Of course not—but maybe there are better ways to do the job. Various strategies have been tried for reducing the influence of verbal/social histories. Might Hefferline's (1962) methods, for example, prove effective? His strategy was to use such small-amplitude responses (minuscule muscle twitches) that their occurrence could not normally be detected—even by the adult human subjects themselves—except by electrophysiological amplifying and recording equipment. With this preparation Hefferline and his colleagues were able to demonstrate operant conditioning without awareness (Hefferline, Keenan, & Harford, 1959) and the necessity of additional contingencies to generate awareness (Hefferline & Perera, 1963). Alternatively, it might be possible to develop new techniques to assess response probability that are not based on response rate and that could be used in situations where response rate is constrained. We have no illusions, however, that researchers can easily create such techniques; indeed, "productive experimental situations are hard to find" (Skinner, 1950, p. 195).

Of course, one can focus on principles other than those that describe how variables affect response probability. Whether or not the complexities of the human Skinner box preparation will be problem-

atic depends on the specific questions being asked. Preparations where humans get points by pressing buttons might, in fact, be very effective for addressing certain kinds of questions even though *rate* of button pressing by humans might not be very revealing about fundamental effects on response probability. The advantages and disadvantages would need to be considered in each case relative to the specific experimental question.

WHAT DOUBTS AND CONCERNS OF NONBEHAVIORAL PSYCHOLOGISTS ARE TO BE ADDRESSED?

Although some nonbehavioral psychologists might question whether our basic behavioral processes operate in human behavior, many do not. What they often do, however, is regard such processes as "primitive"—that is, as automatic as opposed to deliberate processes or as procedural as opposed to declarative memory. They acknowledge that the "primitive" processes operate and even dominate under some conditions (Anderson, 1980; Lindsay & Norman, 1972; Schwartz & Lacey, 1982, 1988; Squire, 1987, pp. 151–174). But the "primitive" processes are not the ones that they find most interesting. Furthermore, even the "primitive" processes are often described in cognitive/information-processing terms (e.g., Davey, 1988; Dickenson, 1980; Schwartz & Lacey, 1982). Behavior analysts, of course, are unlikely to find such interpretations useful. Nonetheless, it is not at all obvious what particular empirical results would force such interpretations to be rejected. We wonder, therefore, what leads Baron et al. to think that demonstrating similarities in the behavioral relationships between humans and nonhumans in a Skinner-box-type apparatus will have much impact on the thinking of nonbehavioral psychologists.

From the perspective of behavior analytic theory, many of the phenomena studied by nonbehavioral psychologists are complex, composite, higher-order phenomena rather than fundamental. For example, from a behavior analytic perspective, verbal behavior and remembering do not exemplify fundamental processes but are, instead, interpreted as products of combinations of fundamental relations involving stimulus control, deprivation, differential reinforcement, and so forth. Behavior analysts have appropriately focused their basic research on those fundamental relations. The principles so derived are expected to apply broadly across phenomena. One consequence, however, is that basic behavior-analytic research often has the *surface* appearance of being remote from the study of phenomena like remembering, problem-solving, creativity, and language. Indeed, since nonbehavioral psychologists study such phenomena directly and explicitly, many people conclude (mistakenly) that such phenomena are the purview of nonbehavioral psychologists.

Behavior analysts could contribute more visibly to the understanding of such complex human phenomena, and there would be advantages in doing so. For one thing, such phenomena are important to those who have to deal with human behavior on a practical level. Also, such phenomena seem good candidates for testing the effectiveness of behavior-analytic conceptual analyses (interpretations). First, some interesting phenomenon—perhaps the results of an experiment conducted and interpreted from a nonbehavioral perspective—could be reinterpreted in behavior-analytic terms. The conceptual analysis is likely to suggest certain novel manipulations which could then be implemented in an experiment. The experiment might generate data that support a reinterpretation and more effective understanding of the original phenomenon. For example, the experiment might show that phenomena which are treated as members of distinct classes within a nonbehavioral conceptual system are actually points along a continuum of effects. A result of this sort would be a tangible benefit of a behavior-analytic interpretation, of interest to anyone concerned with understanding the phenomenon regardless of his or her theoretical orientation.

One general strategy has been to develop experimental analogs (with humans or nonhumans) of complex human phenomena. Although research of this type should not be confused with research designed to determine the forms of fundamental relationships, it can, nevertheless, move us beyond interpretation. It is important, however, to do more than simply demonstrate that the analogous performance occurs. As Catania (1983) noted:

> If producing an analog were the only function of the synthesis, . . . a thought experiment would do as well. Instead, once a phenomenon has been demonstrated by a behavior synthesis, its defining properties and its range of applicability can be refined by subsequent research. The success of the synthesis is then judged not only on the basis of the empirical results but also on the extent to which the refined understanding of the phenomenon has implications for the human non-laboratory situations from which the analog emerged. (p. 59)

To suggest greater flexibility in the kinds of experimental preparations used with humans is not at all equivalent to suggesting abandonment of the traditional behavior analytic approach (cf. Sidman, 1990). Nothing inherent in operant principles nor in traditional behavior-analytic research methodology requires that the procedure mimic features of the rat's lever-box. It is not the apparatus that makes an experiment (with humans or with nonhumans) an operant experiment. Just as the results from the Skinner Box (human or nonhuman) can be interpreted in cognitive/information-processing terms, so too virtually any experiment on human behavior can be legitimately construed as a study of human operant behavior. This is true of studies of remembering, problem-solving, transfer of training, perception, and scientific creativity.

Perhaps it is relevant to recall that Skinner's early research on verbal behavior included counting frequencies of alliteration in Shakespeare's sonnets (Skinner, 1939) and recording verbal behavior evoked by an auditory form of projective test (Skinner, 1936); that Keller and Schoenfeld's (1950) book made reference to a wide range of experimental techniques, although with due critical regard to their analytic usefulness; and that Keller's (1958) research that challenged contemporary views about the learning process was on the learning of Morse code.

In short, our concern has been that some behavior-analytic research on human behavior seems prompted more by features of an experimental preparation than by broad questions about the determinants of human behavior. If so, the field of human operant behavior probably is making less progress than it otherwise could.

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